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Joshua Lederberg
UNIVERSITY PROFESSOR

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Dr. Marina Chicurel
Dept. Neurobiology
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220 Longwood Ave.
Boston MA 02115

Dear Dr. Chicurel:

Thank you for your ms. on plasmids and gene flux. I found it very well written, thoughtful (except for your remarks about "great scientists") and accurate; and I have very few, and only minor comments.

A couple of mis-spellings: pilli should be pili; xilanase => xylanase.

-- re Figure Legends. We could have some further discussions about plasmids being "merely" accessory. I think that is mostly an artefact of how we find them. It is easy to construct situations where the plasmid contains genes necessary for viability, and I am sure this will be found in nature as well. I agree, that essential plasmid genes are likely to become integrated into the chromosome. So accessory is a tendency rather than a categorical attribute.

Likewise, there may be exceptions to there being just a single chromosome. Some megaplasmids are so large, there becomes a question of definition. But I would just put a qualifier "usually" or "almost always".

You have given the traditional story of "skepticism" about Griffith and then Avery. I think Griffith was mostly ignored -- the bacteriologists had no genetic background to think very deeply about the implications of his story; and the geneticists were hardly aware of it. (Dobzhansky does mention it as "induced mutation" in Genetics and the Origin of Species.) The skepticism about Avery was whether they had really proven that their active material was just DNA, and this was a matter of lively (and appropriate) controversy for 8 or 10 years after 1944. It would be a long story to go into all the nuances of how theories fight their way into general acceptance, and you may not want to bother going beyond Wyatt and Stent's "founders' myths", to borrow a phrase from Jan Sapp, for the purposes of your article.

There are of course many other examples of suspected horizontal transfer. You are probably well acquainted with most of them, but enclosed are some I've collected recently.

You did not send me the "bibliography" or further reading list. The most forceful exposition, which I found very appealing, is the book:

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Aa Sonea, Sorin
Ab Panisset, Maurice
TI A new bacteriology.
CL 140 p.
PP Boston MA: Jones and Bartlett.
DA 1983.

You probably had in mind to mention Margulis' -- a new edition is just coming out:

Lynn Margulis -- Symbiosis in Cell Evolution: microbial ~~evolution since~~ communities in the Archean and Proterozoic eons. 2d Ed. New York: Freeman & Co., 1993

In fact, Lynn asked me to write a critical foreword; I enclose that for your interest. (by mail.)

Sincerely,

John Ludwig